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ARTICLE I.

Obſervations on the Baſaltic Coaſt of Ireland. In a Letter from WM. RICHARDSON, *D. D. to the Honourable* GEO. KNOX, *M. P. Communicated by Mr. Knox.*

DEAR SIR,

WHEN you left Portruſh in Auguſt laſt, you lamented that you could not accompany me on two expeditions I was then preparing for, one to explore our coaſt in ſearch of the WHYNN DYKES (we call them GAWS) which iſſuing from it at right angles proceed nearly due northward, and croſs moſt of the Hebrides, cutting through all ſubſtances indifferently.

I found four at BENGORE HEAD, in addition to the two at the Giant's Cauſeway we examined together, and whoſe internal priſmatic conſtruction, and other circumſtances aſtoniſhed you ſo much; I found alſo four at FAIRHEAD, making ten in all, every one of which that I could get acceſs to, differed in ſome material points from all the others.

Mr. Mills (Phil. Tranſ. 1790,) mentions ſeveral of theſe WHYNN DYKES he met with in the Scotch iſlands, but he does not ſeem to have examined them with attention, or noticed their differences, if they really differ from each other as ours do.

As ſoon as I am able to explore the coaſt from DUNSCURICH to BALLINTAY, where poſſibly I may find more, I will (according to my promiſe) give you an accurate account

of theſe wonderful walls, which ſeem to me utterly irreconcileable to any of the theories invented by coſmogoniſts.

In this Letter I will confine myſelf to the mighty promontory of FAIRHEAD, which forms the north-eaſt point of our iſland, and a line drawn from it to Cape Clear in Kerry meaſures its greateſt dimenſion.

Before I proceed to particulars, I muſt dwell a little on its magnificence, both becauſe you yourſelf, when admiring from the water the vaſt baſaltic precipices of BENGORE, (370 feet high) ſeemed to doubt me, when I told you that at FAIRHEAD they were far more ſtupendous; and alſo becauſe Mr. PENNANT aſſerts, that the baſaltic pillars at STAFFA *far exceed the Iriſh in grandeur.*

It has of late been much the faſhion to prefer STAFFA to any thing our baſaltic coaſt can boaſt of, with what foundation will ſoon appear.

In point of grandeur, menſuration affords a good teſt, and fortunately we have the heights of the pillars and precipices at STAFFA accurately taken by Sir Joſeph Banks, and I make myſelf anſwerable for the meaſures I ſtate at FAIRHEAD.

In Staffa the longeſt pillar or range of pillars is 55 feet, and the higheſt point of the precipice and iſland 126; (this laſt aſſertion is from recollection) whereas at Fairhead the height of the face of the columnar precipice, that is, the height of each pillar in a range of near an Engliſh mile, varies only from 252 to 250 feet, and from the angle (ſee view of Faiſhead taken at four miles diſtance W. S. W.) to the level of the ſea, the perpendicular height of the precipitous declivity is at leaſt 300 feet, making together the height of the promontory 550 feet.

After comparing theſe meaſures, it is mortifying to obſerve the animation with which Sir Joſeph Banks deſcribes the magnificence of Staffa, while Fairhead remains unnoticed.

I apprehend it is by far the moſt ſuperb colonade of baſaltic pillars yet diſcovered, though Mr. KIRWAN, on the authority of Mr. Charpentier ſays, the pillars at STOLPE are 300 feet high, but the Encyclopedie tells us, theſe ſame pillars at STOLPE are but 17 or 18, and Gentlemen (particularly my friend Archdeacon Lovel) who have viſited STOLPE, aſſure me that the latter dimenſions ſeem to them the true ones.

The vaſt perpendicular precipice which lines the upper part of Fairhead promontory, is twice cut into by very acute angles; the weſternmoſt of theſe is as it were biſected by a mighty wall, compoſed of pillars near 150 feet long each, while the eaſternmoſt affords a path by which I deſcended to the baſes of the pillars; as I ſtood at the opening of this angle the ſcene about me was the moſt ſtupendous I ever beheld; on my right and left hand enormous walls reaching almoſt to the ſky, intirely columnar, the diameter of each pillar exceeding two feet, but quite out of proportion to their enormous height.

The view below me was terrific, for I ſtood on the top of a ſecond ſtratum of perpendicular pillars, probably 80 feet long each, with the declivity from the bottom of theſe pillars to the ſea, ſteep and precipitous, not much leſs than 300 feet.

The

The views from the ſummit of this promontory are highly gratifying to thoſe who delight in the ſublime, but it is by coaſting its baſe alone that the naturaliſt will obtain entertainment or information; this however is an expedition of difficulty and uncertainty, from the rapidity of the tide that runs cloſe to this headland, and the violent ſurf which generally beats againſt its baſe; twice I reached the north-weſt point of this formidable head without daring to proceed, the third time I was more fortunate, and amply repaid for my perſeverance.

While at Fairhead nature ſeems to have exerted her utmoſt powers in diſplaying a magnificence unrivalled any where elſe, at the ſame time ſhe appears in the conſtruction of this promontory to have adopted different principles from thoſe ſhe has hitherto been governed by in the formation of the reſt of our baſaltic country.

I had always obſerved that our ſtrata were in the ſame place, all ſteadily parallel to each other, and I ſhewed you that this rule obtained in all the precipices we rowed along together; but at Fairhead the caſe is different, for there the columnar ſtrata are inclined to each other in a conſiderable angle.

I alſo ſhewed you, that in every ſeparate ſtratum the material was uniform through its whole extent, the principle of conſtruction, and the degree of perfection with which it was executed, the ſame.

At Fairhead on the contrary, the material is ſenſibly changed, in paſſing from the weſt to the eaſt ſide, the perfection in the workmanſhip of the pillars conſiderably abated, and a new principle of internal conſtruction introduced.

A moſt extraordinary circumſtance alſo occurs here, which I have not met with in any other of the columnar groups with which our baſaltic country abounds, nor do I find any thing ſimilar mentioned by the writers on theſe ſubjects.

In viewing Fairhead from any diſtance, many of its pillars appear to be of a moſt enormous ſize, but upon a near approach thoſe gigantic columns from ten to twenty feet diameter, are found to be formed by an accumulation of the ſmaller ones (from two to three feet) which compoſe the promontory, and adhere together with ſuch force, that when the mighty pillar tumbles down (as ſometimes happens) it breaks in pieces by the violence of a fall 250 feet perpendicular, without ſeparating from each other, the ſmaller pillars of which it is compoſed.

The ſteep declivity is covered quite down to the water edge with enormous fragments, or fruſta of theſe pillars, they are always cylindrical, and to look at their ſides they reſemble an aſſemblage of great beams of timber conſolidated together, while their baſes (generally planes, and at right angles with their axes) exhibit a teſſelated pavement, exactly like the ſurface of the Giant's Cauſeway, but the polygons are ſomewhat larger, and approach more to equilateral figures.

Sometimes theſe ſmaller pillars are continued quite up to the top, of their original ſize, at other times when half way up they merge into, and form greater pillars, which are continued to the ſummit, at other times again they merge into the ſolid rock.

It alſo frequently happens that the lower half of columns, both great and ſmall, has fallen down, leaving the upper half apparently unſupported, yet firm in its place; this laſt circumſtance, however, is common in all our baſaltic precipices.

This ſuſpenſion of the upper parts of pillars, and ſtill more the adheſion of their ſides, which I have juſt now ſhewn to be ſo ſtrong as not to yield to the ſhock of a mighty fall, prove, that at leaſt in this place there are no intervals between the pillars.

From analogy I have long ſuſpected this to be the caſe, every variety of our baſalt is of priſmatic conſtruction, and in *all*, the priſms adhere ſo cloſely together as to be ſeparable by the crow or ſledge alone, the columnar is the only one that has intervals between its priſms, and theſe I have little doubt proceed from external cauſes, but it is impoſſible to penetrate ſo far into a range of pillars as to bring the point to certainty; it is by catching at accidental circumſtances that we are enabled to throw any light upon it.

As I coaſted along the foot of the promontory, the magnificence of the ſcene above me baffled all deſcription, the mighty wall which ſtruck me ſo forcibly when on the top of the cliff, was now an inſignificant object, and the angle through which I had deſcended to the foot of the precipice, was diminiſhed almoſt to nothing.

A little to the eaſtward of this angle (called the Grey Man's Bath) we came oppoſite to the place where a conſiderable part of the precipice had fallen down about 25 years ago; its length ſeemed to be near a furlong, and its depth into the face of the rock muſt have exceeded 150 feet; the materials of this immenſe maſs are ſcattered in vaſt fragments piled upon one another from the foot of the perpendicular precipice, quite down to the water edge, forming by their accumulations a wild variety of caverns, arches, and paſſages beneath the ruins.

The face of rocks which is now laid bare, and which before the diſruption was far in the interior, is by no means ſo columnar as the reſt of the ſurface; ſometimes the vaſt pillars alone are viſible, ſometimes the rocks ſeem quite plain, and in one place the lines ſeparating three or four pillars after aſcending about 150 feet, faintly marked, make a ſlight curve for a few feet, and then merge in the ſolid rock.

All this looks as if the columnar conſtruction did not reach far into the interior of our precipices; but excluſive of the unfairneſs of drawing a general concluſion from this ſolitary inſtance, I muſt admit I could not get ſo near the baſe of the perpendicular face, as to make any accurate obſervations; beſides, the material ſeems to undergo a conſiderable change in the paſſage from the weſt to the eaſt ſide of Fairhead, to have aſſumed a darker colour, and from many of the fragments looks as if compoſed of ſmall trapezoidal priſms, like the Gaw, or Whynn Dyke we examined together at PORT SPAGNA BENGORE, the internal conſtruction of whoſe maſſive horizontal priſms was diſcloſed to us by the nervous arm, and weighty ſledge of your friend Mr. Crampton, without whoſe powerful aſſiſtance we ourſelves would in vain have attempted to diſcover it.

I have not met with any writer who notices this curious circumſtance attending many of our baſalts, to wit, that excluſive of their external priſmatic form, they have, as it were, a ſubordinate

subordinate internal principle of construction, the great prisms breaking into smaller; columnar basalt has rarely this property, I have found it only in the pillars I shewed you at Ballylagan) and at the east side of Fairhead, but all the gaws or dykes seem to be constructed upon this principle, and the two strata which alternate so frequently at Portrush possess the same property in an high degree, and one of these you and your friend Mr. Chenevix have proved by analysing it, that it is pure basalt.

To return to Fairhead, immediately below the Grey Man's Bath, a new stratum of columnar basalt begins to appear, and contrary to what I have observed every where else, not parallel to the one above it (which seems nearly horizontal) but dipping to the south-east, of course as we advance in this direction these strata diverge from each other, and the increasing space between them is fitted up with new materials, to wit, alternate strata of free stone and coal.

These strata are all parallel to the lower columnar stratum; the one immediately resting upon it is coal, this is the vein that was last wrought at MURTOGH, and the adit to the mine runs in along the heads of the basalt pillars.

On tracing these alternate strata in a western direction, they are found to terminate successively against the bases of the upper columnar stratum, always making with it an angle equal to that of the inclination of the two great strata.

Here we have probably the strongest instances that have yet occurred any where of the contacts of coal with columnar basalt, for while the lowest stratum of coal rests with its whole surface on the heads of basalt pillars, every stratum of coal above it comes by regular succession into contact with the bases of the pillars of the upper columnar stratum. I must confess I did not examine these contacts, the acclivity was so steep, and so strewed with enormous masses of rock as to be inaccessible at least to me.

You must observe that the strata at FAIRHEAD totally differ both in materials and arrangement, from the strata composing BENGORE HEAD, which we counted together, and found to be 16 in number; in truth no two of our vast precipices, whether hanging over the sea, or in the interior of the country, perfectly (or I may say nearly) resemble each other in these circumstances.

This variety in the stratification of our basalt country, must be a subject of much curiosity, particularly to geologists, and was no doubt considered as such by Mr. WHITEHURST and MILLS, when they promised in their title pages an account of our strata, yet in the body of their works forgot the topic.

If you can find leisure for another excursion so far north next summer, our strata will afford us an ample fund of entertainment, both in trying the theories of cosmogonists by the test of these strata, and in conjecturing from their interruptions and other circumstances, what changes our globe has undergone since its creation.

I am, SIR,

Your very humble Servant,

WILLIAM RICHARDSON, D. D.

Late Fellow of Trinity College, Dublin.

Clonfeele House, MOY, *Oct.* 22, 1801.

Observations

II.

Obfervations on a Mift which prevailed at Maeftricht on the 14th Nivofe, in the Year 8, by CIT. PAISSÉ, *with Remarks by* CIT. PARMENTIER *.

IT is of great importance in the accurate fciences to pay attention to all the circumftances upon which obfervations can be founded. If this part of natural phifofophy were cultivated with the care it deferves, there is no doubt but we fhould be much more intimately acquainted with the influence of meteors on the health of men, which it fo evidently affects. Thofe fogs which are almoft periodically renewed, prefent to the minifters of health an extenfive field of obfervation, from which it fhould appear, that the moft ufeful confequences refpecting the origin of difeafes may be deduced.

The mift which forms the fubject of the prefent communication was fo peculiar in its appearance and effects, that I have concluded that it might be of advantage to give a detailed account of the fame.

On the morning of the 11th Nivofe Reaumur's thermometer had defcended to $13\frac{1}{2}°$ below O, where it remained for moft part of the day, with no greater variation than $\frac{1}{2}$ a degree. At feven in the evening it was conftant at the fame temperature, and the following day at feven in the morning it had rifen two degrees above the freezing point, and at eleven o'clock the thaw had unequivocally taken place at the temperature of 5 degrees. At two in the evening the thermometer marked $6\frac{1}{2}$ degrees, the thaw continuing; about five in the evening a flight mift appeared, which continued till half paft feven; at eight it was intirely diffipated, and the moon fhone bright, at which time the thermometer defcended again within four degrees of the freezing point; at nine its elevation did not exceed one degree, when the fnow began to harden. On the 13th at feven in the morning the thermometer ftood at feven degrees above O; at nine, eight degrees, and at noon it rofe to the ninth degree, and the fun which appeared from time to time in the courfe of the day had confiderable force. The thermometer kept at the fame elevation, and the thaw continued its progrefs at eight in the evening.

On the 14th day of the month the horizon was covered with a mift of inconfiderable denfity. It appeared like an evaporation from the earth arifing about a metre above the furface. At nine it rofe to the height of feveral metres without becoming lefs denfe; at eleven it became more confiderable; and at noon it was fo thick that two perfons at a very fmall diftance afunder could not fee each other without much difficulty. At three in the evening its denfity was ftill greater, and its fmell fo fetid as ftrongly to impede refpiration. The thermometer during this time underwent no greater change than half a degree, not paffing beyond the ninth degree. The fmell and acrid effects of this meteor were fo dif-

‡ Annales de Chimie, XXXIII. 217.

agreeable,

agreeable, that I thought for a time that this property was communicated by the vapours of the bituminous combustible which is used in this country, and I did not give up my opinion till I had found myself as strongly affected with it in the open country as in the town. My eyes were irritated so as to flow with involuntary tears. I returned home after experiencing the greatest difficulties, and observing that the fog in the streets was much more elevated than in the open country. I observed, as Citizen Fourcroy did on a former occasion at Paris, that it was more easy to find the way in the narrow streets, than in such as were wider, in the former of which the fog descended with much less rapidity.

III.

On the Mechanism of the Eye. By THOMAS YOUNG, *M.D. F.R.S.*

(Continued from page 303.)

HITHERTO I have endeavoured to shew the inconveniences attending other suppositions, and to remove the objections to the opinion of an internal change of the figure of the lens. I shall now state two experiments, which, in the first place, come very near to a mathematical demonstration of the existence of such a change, and, in the second, explain in great measure its origin, and the manner in which it is effected.

I have already described the appearance of the imperfect image of a minute point at different distances from the eye, in a state of relaxation. For the present purpose, I will only repeat, that if the point is beyond the furthest focal distance of the eye, it assumes that appearance which is generally described by the name of a star, the central part being considerably the brightest. (Plate XV. Fig. 36—39.) But, when the focal distance of the eye is shortened, the imperfect image is of course enlarged; and, besides the necessary consequence, the light is also very differently distributed; the central part beomes faint, and the margin strongly illuminated, so as to have almost the appearance of an oval ring. (Fig. 41.) If I apply the slider of the optometer, the shadows of the slits, while the eye is relaxed, are perfectly straight, dividing the oval either way into parallel segments: (Fig. 42, 44.) but, when the accomodation takes place, they immediately become curved, and the more so the further they are from the centre of the image, to which their concavity is directed. (Fig. 43, 45.) If the point be brought much within the focal distance, the change of the eye will increase the illumination of the centre, at the expense of the margin. The same appearances are equally observable, when the effect of the cornea is removed by immersion in water; and the only imaginable way of accounting for the diversity, is to suppose the central parts of the lens to acquire a greater degree of curvature than the marginal parts. If the refraction of the lens remained the same, it is absolutely impossible that any change of the distance of the retina should produce a curvature in those shadows, which, in the relaxed

relaxed ſtate of the eye, are found to be in all parts ſtraight; and, that neither the form nor the relative ſituation of the cornea is concerned, appears from the application of water already mentioned.

The truth of this explanation is fully confirmed by the optometer. When I look through four narrow ſlits, without exertion, the lines always appear to meet in one point: but, when I make the interſection approach me, the two outer lines meet conſiderably beyond the inner ones, and the two lines of the ſame ſide croſs each other at a ſtill greater diſtance. (Plate XV. Fig. 24.)

The experiment will not ſucceed with every eye; nor can it be expected that ſuch an imperfection ſhould be univerſal: but one caſe is ſufficient to eſtabliſh the argument, even if no other were found. I do not however doubt, that in thoſe who have a large pupil, the aberration may be very frequently obſervable. In Dr. Wollaſton's eye, the diverſity of appearance is imperceptible; but Mr. Konig deſcribed the interſection exactly as they appear to me, although he had received no hint of what I had obſerved. The lateral refraction is the moſt eaſily aſcertained, by ſubſtituting for the ſlits a tapering piece of card, ſo as to cover all the central parts of the pupil, and thus determining the neareſt croſſing of the ſhadows tranſmitted through the marginal parts only. When the furtheſt interſection was at 38, I could bring it to 22 parts with two narrow ſlits; but with the tapered card only to 29. From theſe data we may determine pretty nearly, into what form the lens muſt be changed, ſuppoſing both the ſurfaces to undergo proportional alterations of curvature, and taking for granted the dimenſions already laid down: for, from the lateral aberration thus given, we may find (by Prop. III.) the ſubtangents at about one-tenth of an inch from the axis; and the radius of curvature at each vertex, is already determined to be about 21 and 15 hundredths of an inch. Hence the anterior ſurface muſt be a portion of a hyperboloid, of which the greater axis is about 50; and the poſterior ſurface will be nearly parabolical. In this manner the change will be effected, without any diminution of the tranſverſe diameter of the lens. The elongation of its axis will not exceed the fiftieth of an inch; and, on the ſuppoſition with which we ſet out, the protruſion will be chiefly at the poſterior vertex. The form of the lens thus will be nearly that of Plate XV. Fig. 26; the relaxed ſtate being nearly as repreſented in Fig. 25. Should, however, the rigidity of the internal parts, or any other conſiderations, render it convenient to ſuppoſe the anterior ſurface more changed, it would ſtill have room, without interfering with the uvea; or it might even force the uvea a little forwards, without any viſible alteration of the external appearance of the eye.

From this inveſtigation of the change of the figure of the lens, it appears that the action which I formerly attributed to the external coats, cannot afford an explanation of the phenomenon. The neceſſary effect of ſuch an action would be, to produce a figure approaching to that of an oblate ſpheroid; and, to ſay nothing of the inconvenience attending a diminution of the diameter of the lens, the lateral refraction would be much more increaſed than the central; nor would the ſlight change of denſity, at an equal diſtance from the axis,

axis, be at all equivalent to the increafe of curvature: we muft therefore fuppofe fome different mode of action in the power producing the change. Now, whether we call the lens a mufcle or not, it feems demonftrable, that fuch a change of figure takes place as can be produced by no external caufe; and we may at leaft illuftrate it by a comparifon with the ufual action of mufcular fibres. A mufcle never contracts, without at the fame time fwelling laterally, and it is of no confequence which of the effects we confider as primary. I was induced, by an occafional opacity, to give the name of membranous tendons to the radiations from the centre of the lens; but, on a more accurate examination, nothing really analogous to tendon can be difcovered. And, if it were fuppofed that the parts next the axis were throughout of a tendinous, and therefore unchangeable nature, the contraction muft be principally effected by the lateral parts of the fibres; fo that the coats would become thicker towards the margin, by their contraction, while the general alteration of form would require them to be thinner; and there would be a contrariety in the actions of the various parts. But, if we compare the central parts of each furface to the belly of the mufcle, there is no difficulty in conceiving their thicknefs to be immediately increafed, and to produce an immediate elongation of the axis, and an increafe of the central curvature; while the lateral parts co-operate more or lefs, according to their diftance from the centre, and in different individuals in fomewhat different proportions. On this fuppofition, we have no longer any difficulty in attributing a power of change to the cryftalline of fifhes. M. Petit, in a great number of obfervations, uniformly found the lens of fifhes more or lefs flattened: but, even if it were not, a flight extenfion of the lateral part of the fuperficial fibres would allow thofe fofter coats to become thicker at each vertex, and to form the whole lens into a fpheroid fomewhat oblong; and here, the lens being the only agent in refraction, a lefs alteration than in other animals would be fufficient. It is alfo worthy of inquiry, whether the ftate of contraction may not immediately add to the refractive power. According to the old experiment, by which Dr. Goddard attempted to fhow that mufcles become more denfe as they contract, fuch an effect might naturally be expected. That experiment is, however, very indecifive, and the opinion is indeed generally exploded, but perhaps too haftily; and whoever fhall afcertain the exiftence or non-exiftence of fuch a condenfation, will render effential fervice to phyfiology in general.

Dr. Pemberton, in the year 1719, firft fyftematically difcuffed the opinion of the mufcularity of the cryftalline lens *. He referred to Leeuwenhoek's microfcopical obfervations; but he fo overwhelmed his fubject with intricate calculations, that few have attempted to develope it: and he grounded the whole on an experiment borrowed from Barrow, which with me has totally failed; and I cannot but agree with Dr. Olbers in the remark, that it is eafier to confute him than to underftand him. He argued for a partial change of the figure of the lens; and perhaps the opinion was more juft than the reafons

* De Facultate Oculi qua ad diverfas Rerum diftantias fe accommodat. L. B. 1719. Ap. Hall. Difp. Anat. IV. p. 301.

 adduced

adduced for its support. Lobé, or rather Albinus*, decidedly favours a similar theory; and suggests the analogy of the lens to the muscular parts of pellucid animals, in which even the best microscopes can discover no fibres. Camper also mentions the hypothesis with considerable approbation †. Professor Reil published, in 1793, a Dissertation on the Structure of the Lens; and, in a subsequent paper, annexed to the translation of my former Essay in Professor Gren's Journal ‡, he discussed the question of its muscularity. I regret that I have not now an opportunity of referring to this publication; but I do not recollect that Professor Reil's objections are different from those which I have already noticed.

Considering the sympathy of the crystalline lens with the uvea, and the delicate nature of the change of its figure, there is little reason to expect that any artificial stimulus would be more successful in exciting a contractive action in the lens, than it has hitherto been in the uvea; much less would that contraction be visible without art. Soon after Mr. Hunter's death, I pursued the experiment which he had suggested, for ascertaining how far such a contraction might be observable. My apparatus (Plate XV. Fig. 27.) was executed by Mr. Jones. It consisted of a wooden vessel blacked within, which was to be filled with cool, and then with warmer water: a plane speculum was placed under it; a perforation in the bottom was filled with a plate of glass; proper rings were fixed for the reception of the lens, or of the whole eye, and also wires for transmitting electricity: above these, a piece of ground and painted glass, for receiving the image, was supported by a bracket, which moved by a pivot, in connection with a scale divided into fiftieths of an inch. With this apparatus I made some experiments, assisted by Mr. Wilkinson, whose residence was near a slaughter-house: but we could obtain, by this method, no satisfactory evidence of the change; nor was our expectation much disappointed. I understand also, that another member of this Society was equally unsuccessful, in attempting to produce a conspicuous change in the lens by electricity.

XI. In man and in the most common quadrupeds, the structure of the lens is nearly similar. The number of radiations is of little consequence; but I find that in the human crystalline there are ten on each side, (Plate XV. Fig. 46.) not three, as I once, from a hasty observation, concluded §. Those who find any difficulty in discovering the fibres, must have a sight very ill adapted to microscopical researches. I have laboured with the most obstinate perseverance to trace nerves into the lens, and I have sometimes imagined that I had succeeded; but I cannot positively go further than to state my full conviction of their existence, and of the precipitancy of those who have absolutely denied it. The long nerves, which are very conspicuous between the choroid and sclerotic coats, divide each into two,

* De quibusdam Oculi Partibus, L. B. 1746. Ap. Hall. Disp. Anat. IV. p. 301.

† De Oculo Humano. L. B. 1742. Ap. Hall. Disp. Anat. VII. 2. p. 108, 109.

‡ 1794. p. 352, 354.

§ De Corp. Hum. Vir. Conf. p. 68.

three,

three, or more branches, at the ſpot where the ciliary zone begins, and ſeem indeed to furniſh the choroid with ſome fine filaments at the ſame place. The branches often re-unite, with a ſlight protuberance, that ſcarcely deſerves the name of a ganglion: here they are tied down, and mixed with the hard whitiſh-brown membrane that covers the compact ſpongy ſubſtance, in which the veſſels of the ciliary proceſſes anaſtomoſe and ſubdivide. (Plate XV. Fig. 47.) The quantity of the nerves which proceeds to the iris, appears to be conſiderably ſmaller than that which arrives at the place of diviſion: hence there can be little doubt that the diviſion is calculated to ſupply the lens with ſome minute branches; and it is not improbable, from the appearance of the parts, that ſome fibres may paſs to the cornea; although it might more naturally be expected, that the tunica conjunctiva would be ſupplied from without. But the ſubdiviſions which probably paſs to the lens, enter immediately into a mixture of ligamentous ſubſtance, and of a tough browniſh membrane; and I have not hitherto been able to develope them. Perhaps animals may be found in which this ſubſtance is of a different nature; and I do not deſpair that, with the aſſiſtance of injections, for more readily diſtinguiſhing the blood veſſels, it may ſtill be poſſible to trace them in quadrupeds. Our inability to diſcover them, is ſcarcely an argument againſt their exiſtence: they muſt naturally be delicate and tranſparent; and we have an inſtance, in the cornea, of conſiderable ſenſibility, where no nerve has yet been traced. The capſule adheres to the ciliary ſubſtance, and the lens to the capſule, principally in two or three points; but I confeſs, I have not been able to obſerve that theſe points are exactly oppoſite to the trunks of nerves; ſo that, probably, the adheſion is chiefly cauſed by thoſe veſſels which are ſometimes ſeen paſſing to the capſule in injected eyes. We may, however, diſcover ramifications from ſome of theſe points, upon and within the ſubſtance of the lens, (Plate XV. Fig. 48.) generally following a direction near to that of the fibres, and ſometimes proceeding from a point oppoſite to one of the radiating lines of the ſame ſurface. But the principal veſſels of the lens appear to be derived from the central artery, by two or three branches at ſome little diſtance from the poſterior vertex; which I conceive to be the cauſe of the frequent adheſion of a portion of a cataract to the capſule, about this point: they follow nearly the courſe of the radiations, and then of the fibres; but there is often a ſuperficial ſubdiviſion of one of the radii, at the ſpot where one of them enters. The veſſels coming from the choroid appear principally to ſupply a ſubſtance, hitherto unobſerved, which fills up the marginal part of the capſule of the cryſtalline, in the form of a thin zone, and makes a ſlight elevation, viſible even through the capſule, (Fig. 49—51.) It conſiſts of coarſer fibres than the lens, but in a direction nearly ſimilar; they are often intermixed with ſmall globules. In ſome animals, the margin of the zone is crenated, eſpecially behind, where it is ſhorter: this is obſervable in the partridge; and, in the ſame bird, the whole ſurface of the lens is ſeen to be covered with points, or rather globules, arranged in regular lines, (Plate XV. Fig. 52.) ſo as to have ſomewhat the appearance of a honeycomb, but towards the vertex leſs uniformly diſpoſed. This regularity is a ſufficient proof that there could be no optical deception in the appear-

 ance;

ance; although it requires a good microscope to discover it distinctly: but the zone may be easily peeled off under water, and hardened in spirits. Its use is uncertain; but it may possibly secrete the liquid of the crystalline; and it as much deserves the name of a gland, as the greater part of the substances usually so denominated. In peeling it off, I have very distinctly observed ramifications, which were passing through it into the lens; (Plate XV. Fig. 50.) and indeed it is not at all difficult to detect the vessels connecting the margin of the lens with its capsule; and it is surprising that M. Petit should have doubted of their existence. I have not yet clearly discerned this crystalline gland in the human eye; but I infer the existence of something similar to the globules, from the spotted appearance of the image of a lucid point already mentioned; for which I can no otherwise account, than by attributing it to a derangement of these particles, produced by the external force, and to an unequal impression made by them on the surface of the lens.

In birds and in fishes, the fibres of the crystalline radiate equally, becoming finer as they approach the vertex, till they are lost in a uniform substance, of the same degree of firmness, which appears to be perforated in the centre by a blood vessel. (Plate XV. Fig. 53.) In quadrupeds, the fibres at their angular meeting are certainly not continued, as Leeuwenhoek imagined, across the line of division; but there does not appear to be any dissimilar substance interposed between them, except that very minute trunks of vessels often mark that line. But, since the whole mass of the lens, as far as it is moveable, is probably endued with a power of changing its figure, there is no need of any strength of union, or place of attachment, for the fibres, since the motion meets with little or no resistance. Every common muscle, as soon as its contraction ceases, returns to its natural form, even without the assistance of an antagonist; and the lens itself, when taken out of the eye, in its capsule, has elasticity enough to re-assume its proper figure, on the removal of a force that has compressed it. The capsule is highly elastic; and, since it is laterally fixed to the ciliary zone, it must co-operate in restoring the lens to its flattest form. If it be inquired, why the lens is not capable of becoming less convex, as well as more so, it may be answered, that the lateral parts have probably little contractive power; and, if they had more, they would have no room to increase the size of the disc, which they must do, in order to shorten the axis; and the parts about the axis have no fibres so arranged as to shorten it by their own contraction.

I consider myself as being partly repaid for the labour lost in search of the nerves of the lens, by having acquired a more accurate conception of the nature and situation of the ciliary substance. It had already been observed, that in the hare and in the wolf, the ciliary processes are not attached to the capsule of the lens; and if by the ciliary processes we understand those filaments which are seen detached after tearing away the capsule, and consist of ramifying vessels, the observation is equally true of the common quadrupeds; and I will venture to say, of the human eye*. Perhaps this remark has been made by others,

* Vid. Hall. Physiol. V. p. 432. et Duverney, ibi citat.

but

but the circumſtance is not generally underſtood. It is ſo difficult to obtain a diſtinct view of theſe bodies, undiſturbed, that I am partly indebted to accident, for having been undeceived reſpecting them: but, having once made the obſervation, I have learnt to ſhew it in an unqueſtionable manner. I remove the poſterior hemiſphere of the ſclerotica, or ſomewhat more, and alſo as much as poſſible of the vitreous humour, introduce the point of a pair of ſciſſors into the capſule, turn out the lens, and cut off the greater part of the poſterior portion of the capſule, and of the reſt of the vitreous humour. I next diſſect the choroid and uvea from the ſclerotica; and, dividing the anterior part of the capſule into ſegments from its centre, I turn them back upon the ciliary zone. The ciliary proceſſes then appear, covered with their pigment, and perfectly diſtinct both from the capſule and from the uvea; (Plate XV. Fig. 54.) and the ſurface of the capſule is ſeen ſhining, and evidently natural, cloſe to the baſe of theſe ſubſtances. I do not deny that the ſeparation between the uvea and the proceſſes, extends ſomewhat further back than the ſeparation between the proceſſes and the capſule; but the difference is inconſiderable, and, in the calf, does not amount to above half the length of the detached part. The appearance of the proceſſes is wholly irreconcileable with muſcularity; and their being conſidered as muſcles attached to the capſule, is therefore doubly inadmiſſible. Their lateral union with the capſule, commences at the baſe of their poſterior ſmooth ſurface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; ſo that, however this portion of the baſe of the proceſſes were diſpoſed to contract, it would be much too ſhort to produce any ſenſible effect. What their uſe may be, cannot eaſily be determined: if it were neceſſary to have any peculiar organs for ſecretion, we might call them glands, for the percolation of the aqueous humour; but there is no reaſon to think them requiſite for this purpoſe.

The marſupium nigrum of birds, and the horſe-ſhoe-like appearance of the choroid of fiſhes, are two ſubſtances which have ſometimes, with equal injuſtice, been termed muſcular. All the apparent fibres of the marſupium nigrum are, as Haller had very truly aſſerted, merely duplicatures of a membrane, which, when its ends are cut off, may eaſily be unfolded under the microſcope, with the aſſiſtance of a fine hair pencil, ſo as to leave no longer any ſuſpicion of a muſcular texture. The experiment related by Mr. Home *, can ſcarcely be deemed a very ſtrong argument for attributing to this ſubſtance a faculty which its appearance ſo little authoriſes us to expect in it. The red ſubſtance in the choroid of fiſhes, (Plate XV. Fig. 55.) is more capable of deceiving the obſerver; its colour gives it ſome little pretenſion, and I began to examine it with a prepoſſeſſion in favour of its muſcular nature. But, when we recollect the general colour of the muſcles of fiſhes, the conſideration of its redneſs will no longer have any weight. Stripped of the membrane which looſely covers its internal ſurface, (Fig. 56.) it ſeems to have tranſverſe diviſions, ſomewhat reſembling thoſe of muſcles, and to terminate in a manner ſomewhat ſimilar;

* Phil. Tranſ. for 1796, p. 18.

(Fig. 57.) but, when viewed in a microfcope, the tranfverfe divifions appear to be cracks, and the whole mafs is evidently of a uniform texture, without the leaft fibrous appearance; and, if a particle of any kind of mufcle is compared with it, the contraft becomes very ftriking. Befides, it is fixed down, throughout its extent, to the pofterior lamina of the choroid, and has no attachment capable of directing its effect; to fay nothing of the difficulty of conceiving what that effect could be. Its ufe muft remain, in common with that of many other parts of the animal frame, entirely concealed from our curiofity.

The bony fcales of the eyes of birds, which were long ago defcribed in the Philofophical Tranfactions by Mr. Ranby *, and by Mr. Warren †, afterwards in two excellent Memoirs of M. Petit on the eye of the turkey and of the owl ‡, and lately by Mr. Pierce Smith §, and Mr. Home ||, can, on any fuppofition, have but little concern in the accommodation of the eye to different diftances: they rather feem to be neceffary for the protection of that organ, large and prominent as it is, and unfupported by any ftrength in the orbit, againft the various accidents to which the mode of life and rapid motion of thofe animals muft expofe it; and they are much lefs liable to fracture than an entire bony ring of the fame thicknefs would have been. The marfupium nigrum appears to be intended to affift in giving ftrength to the eye, to prevent any change in the place of the lens by external force: it is fo fituated as to intercept but little light, and that little is principally what would have fallen on the infertion of the optic nerve; and it feems to be too firmly tied to the lens, even to admit any confiderable elongation of the axis of the eye, although it certainly would not impede a protrufion of the cornea.

With refpect to the eyes of infects, an obfervation of Poupart deferves to be repeated here. He remarks, that the eye of the libellula is hollow; that it communicates with an air-veffel placed longitudinally in the trunk of the body; and that it is capable of being inflated from this body: he fuppofes that the infect is provided with this apparatus, in order for the accommodation of its eye to the perception of objects at different diftances **. I have not yet had an opportunity of examining the eye of the libellula; but there is no difficulty in fuppofing that the means of producing the change of the refractive powers of the eye, may be, in different claffes of animals, as diverfified as their habits, and the general conformation of their organs.

I beg leave to correct here an obfervation in my former paper, relative to the faint lateral radiations, which I fuppofed to proceed from the margin of the iris ††. I find, on further examination, that they are occafioned by reflections from the eye-lafhes.

XII. I fhall now finally recapitulate the principal objects and refults of the inveftigation which I have taken the liberty of detailing fo fully to the Royal Society. Firft, the deter-

* Phil. Tranf. Vol. XXXIII. p. 223. Abr. Vol. VII. p. 435.

† Phil. Tranf. Vol. XXXIV. p. 113. Abr. Vol. VII. p. 437.

‡ Mém. de l'Acad. 1735, p. 163. 1736, p. 166. Ed. Amft.

§ Phil. Tranf. for 1795, p. 263.

|| Phil. Tranf. for 1796, p. 14.

** Phil. Tranf. Vol. XXII. p. 673. Abr. II. p. 762.

†† Phil. Tranf. for 1793, p. 178.

mination of the refractive power of a variable medium, and its application to the constitution of the crystalline lens. Secondly, the construction of an instrument for ascertaining, upon inspection, the exact focal distance of every eye, and the remedy for its imperfections. Thirdly, to shew the accurate adjustment of every part of the eye, for seeing with distinctness the greatest possible extent of objects at the same instant. Fourthly, to measure the collective dispersion of coloured rays in the eye. Fifthly, by immerging the eye in water, to demonstrate that its accommodation does not depend on any change in the curvature of the cornea. Sixthly, by confining the eye at the extremities of its axis, to prove that no material alteration of its length can take place. Seventhly, to examine what inference can be drawn from the experiments hitherto made on persons deprived of the lens; to pursue the inquiry, on the principles suggested by Dr. Porterfield; and to confirm his opinion of the utter inability of such persons to change the refractive state of the organ. Eighthly, to deduce, from the aberration of the lateral rays, a decisive argument in favour of a change in the figure of the crystalline; to ascertain, from the quantity of this aberration, the form into which the lens appears to be thrown in my own eye, and the mode by which the change must be produced in that of every other person. And I flatter myself, that I shall not be deemed too precipitate, in denominating this series of experiments satisfactorily demonstrative.

EXPLANATION OF THE FIGURES.

Plate XIII. Fig. 1. See page 256. Prop. III.

Fig. 2. See page 256. Prop. IV.

Fig. 3. See page 258. Prop. V.

Fig. 4—6. Relating to the optometer. See page 259.

Plate XIV. Fig. 7. The form of the ends of the optometer, when made of card. The apertures in the shoulders are for holding a lens: the square ends turn under, and are fastened together.

Fig. 8. The scale of the optometer. The middle line is divided, from the lower end, into inches. The next column shews the number of a concave lens requisite for a short-sighted eye; by looking through the slider and observing the number opposite to which the intersection appears when most remote. By observing the place of apparent intersection when nearest, the number requisite will be found in the other column, provided that the eye have the average power of accommodation. At the other end, the middle line is graduated for extending the scale of inches by means of a lens *four* inches in focus; the negative numbers implying that such rays as proceed from them are made to converge towards a point on the other side of the lens. The other column shews the focal length of convex glasses required by those eyes to which the intersection appears, when nearest, opposite the respective places of the numbers.

Fig. 9. A side view of the optometer, half its size.

Fig. 10. The appearance of the lines through the slider.

Fig.

Fig. 11. Method of meaſuring the magnitude of an image on the retina. See page 293.

Fig. 12. Diagonal ſcale drawn on a looking-glaſs.

Fig. 13. The method of applying a lens with water to the cornea.

Fig. 14. The appearance of a ſpectrum occaſioned by preſſure; and the inflection of ſtraight lines ſeen within the limits of the ſpectrum.

Fig. 15. An illuſtration of the enlargement of the image, which would be the conſequence of an elongation of the eye: the images of the candles which, in one inſtance, fall on the inſertion of the nerve, falling, in the other inſtance, beyond it.

Plate XV. Fig. 16. The ſucceſſive forms of the image of a large diſtant object, as it would be delineated by each refractive ſurface in the eye; to ſhew how that form at laſt coincides with the retina. E G is the diſtance between the foci of horizontal and vertical rays in my eye.

Plate XV. Fig. 17. Vertical ſection of my right eye, ſeen from without; twice the natural ſize.

Fig. 18. Horizontal ſection, ſeen from above.

Fig. 19. Front view of my left eye when the pupil is contracted; of the natural ſize.

Fig. 20. The ſame view when the pupil is dilated.

Fig. 21. Outline of the eye and its ſtraight muſcles when at reſt.

Fig. 22. Change of figure which would be the conſequence of the action of thoſe muſcles upon the eye, and upon the adipoſe ſubſtance behind it.

Fig. 23. Scale of the ſmall optometer.

Fig. 24. Appearance of four images of a line ſeen by my eye when its focus is ſhorteſt.

Fig. 25. Outline of the lens when relaxed; from a compariſon of M. Petit's meaſures with the phenomena of my own eye, and on the ſuppoſition that it is found in a relaxed ſtate after death.

Fig. 26. Outline of the lens ſufficiently changed to produce the ſhorteſt focal diſtance.

Fig. 27. Apparatus for aſcertaining the focal length of the lens in water.

Plate XV. Fig. 28. Various forms of the image depicted by a cylindrical pencil of rays obliquely refracted by a ſpherical ſurface, when received on planes at diſtances progreſſively greater.

Fig. 29. Image of a minute lucid object held very near to my eye.

Fig. 30. The ſame appearance when the eye has been rubbed.

Fig. 31—37. Different forms of the image of a lucid point at greater and greater diſtances; the moſt perfect focus being like Fig. 33, but much ſmaller.

Fig. 38. Image of a very remote point ſeen by my right eye.

Fig. 39. Image of a remote point ſeen by my left eye; being more obtuſe at one end, probably from a leſs obliquity of the poſterior ſurface of the cryſtalline lens.

Fig. 40. Combination of two figures ſimilar to the fifth variety of Fig. 28; to imitate Fig. 38.

Fig. 41. Appearance of a diſtant lucid point when the eye is adapted to a very near object.

Fig.

Fig. 42, 44. Shadow of parallel wires in the image of a diſtant point, when the eye is relaxed.

Fig. 43, 45. The ſame ſhadows rendered curved by a change in the figure of the cryſtalline lens.

Fig. 46. The order of the fibres of the human cryſtalline.

Fig. 47. The diviſion of the nerves at the ciliary zone; the ſclerotica being removed. One of the nerves of the uvea is ſeen paſſing forwards and ſubdividing. From the calf.

Fig. 48. Ramifications from the margin of the cryſtalline lens.

Fig. 49. The zone of the cryſtalline faintly ſeen through the capſule.

Fig. 50. The zone raiſed from its ſituation, with the ramifications paſſing through it into the lens.

Fig. 51. The zone of the cryſtalline detached.

Plate XV. Fig. 52. The crenated zone, and the globules regularly arranged on the cryſtalline of the partridge.

Fig. 53. The order of the fibres in the lens of birds and fiſhes.

Fig. 54. The ſegments of the capſule of the cryſtalline turned back, to ſhew the detached ciliary proceſſes. From the calf.

Fig. 55. Part of the choroid of the cod-fiſh, with its red ſubſtance. The central artery hangs looſe from the inſertion of the nerve.

Fig. 56. The membrane covering this ſubſtance internally, raiſed by the blow-pipe.

Fig. 57. The appearance of the red ſubſtance, after the removal of the membrane.

IV.

Experiments on the Chemical Production and Agency of Electricity. By William Hyde Wollaston, *M.D. F.R.S.**

NOTWITHSTANDING the power of Mr. Volta's electric pile is now known to be proportional to the diſpoſition of one of the metals to be oxidated by the fluid interpoſed, a doubt has been entertained by many perſons, whether this power ariſes from the chemical action of the fluid on the metal, or, on the contrary, whether the oxidation itſelf may not be occaſioned by electricity, ſet in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cauſe of the electric phenomena obſerved, is, I think, to be inferred from the following experiments, which exhibit the galvanic proceſs reduced to its moſt ſimple ſtate.

Experiment 1. If a piece of zinc and a piece of ſilver have each one extremity immerſed in the ſame veſſel, containing ſulphuric or muriatic acid diluted with a large quantity of

* Phil. Tranſ. for 1801, p. 427.

water, the zinc is diffolved, and yields hidrogen gas, by decompofition of the water: the filver, not being acted upon has no power of decompofing water; but, whenever the zinc and filver are made to touch, or any metallic communication is made between them, hidrogen gas is alfo formed at the furface of the filver.

Any other metal befide zinc, which by affiftance of the acid employed is capable of decompofing water, will fucceed equally, if the other wire confifts of a metal on which the acid has no effect.

Experiment 2. If zinc, iron or copper, are employed with gold in dilute nitric acid, nitrous gas is formed; in the fame manner, and under the fame circumftances, as the hidrogen gas in the former experiment.

Experiment 3. Experiments analogous to the former, and equally fimple, may alfo be made with many metallic folutions. If, for inftance, the folution contains copper, it will be precipitated by a piece of iron, and appear on its furface. Upon filver merely immerfed in the fame folution, no fuch effect is produced; but as foon as the two metals are brought into contact, the filver receives a coating of copper.

In the explanation of thefe experiments, it is neceffary to advert to a point eftablifhed by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is fufficient to oxidate one of the wires of communication, the wire connected with the oppofite extremity affords hidrogen gas.

Since the extrication of hidrogen, in this inftance, is feen to depend on electricity, it is probable, that in other inftances, electricity may be alfo requifite for its converfion into gas. It would appear, therefore, that in the folution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hidrogen gas, even in that cafe depends on a tranfition of electricity between the fluid and the metal.

We fee, moreover, in the firft experiment, that the zinc, without contact of any other metal, has the power of decompofing water; and we can have no reafon to fuppofe that the contact of the filver produces any new power, but that it ferves merely as a conductor of electricity, and thereby occafions the formation of hidrogen gas.

In the third experiment alfo, the iron by itfelf has the power of precipitating copper, by means, I prefume, of electricity evolved during its folution; and here likewife the filver, by conducting that electrity, acquires the power of precipitating the copper in its metallic ftate.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be feen, that the fame transfer of chemical power, and the fame apparent reverfion of the ufual order of chemical affinities in the precipitation of copper by filver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, confifts of a cylinder feven inches in diameter, with a conductor on each fide, 16 inches long, and $3\frac{1}{2}$ inches diameter,

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diameter, each furniſhed with a ſliding electrometer, to regulate the ſtrength of the ſpark received from them.

Experiment 4. Having a wire of fine ſilver $\frac{1}{120}$ of an inch in diameter, I coated the middle of it for two or three inches, with ſealing wax, and by cutting through in the middle of the wax, expoſed a ſection of the wire. The two coated extremities of the wire thus divided, were immerſed in a ſolution of ſulphate of copper, placed in an electric circuit between the two conductors; and ſparks, taken at $\frac{1}{10}$ of an inch diſtance, were paſſed by means of them through the ſolution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor, had a precipitate formed on its ſurface, which, upon being burniſhed, was evidently copper; but the oppoſite wire had no ſuch coating.

Upon reverſing the direction of the current of electricity, the order of the phenomena was of courſe reverſed; the copper being ſhortly re-diſſolved by aſſiſtance of the oxidating power of poſitive electricity, and a ſimilar precipitate formed on the oppoſite wire.

Experiment 5. A ſimilar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a ſolution of corroſive ſublimate, had the ſame ſucceſs.

The chemical agency, therefore, of common electricity, is thus proved to be the ſame with the power excited by chemical means; but, ſince a difference has been obſerved in the comparative facility with which the pile of Volta decompoſes water, and produces other effects of oxidation and de-oxidation of bodies expoſed to its action, I have been at ſome pains to remove this difficulty, and can at leaſt produce a very cloſe imitation of the galvanic phenomena, by common electricity.

It has been thought neceſſary to employ powerful machines, and large Leyden jars, for the decompoſition of water; but when I conſidered that the decompoſition muſt depend on duly proportioning the ſtrength of the charge of electricity to the quantity of water, and that the quantity expoſed to its action at the ſurface of communication depends on the extent of that ſurface, I hoped that, by reducing the ſurface of communication, the decompoſition of water might be effected by ſmaller machines, and with leſs powerful excitation, than have hitherto been uſed for that purpoſe; and, in this hope, I have not been diſappointed.

Experiment 6. Having procured a ſmall wire of fine gold, and given it as fine a point as I could, I inſerted it into a capillary glaſs tube; and after heating the tube, ſo as to make it adhere to the point and cover it in every part, I gradually ground it down, till, with a pocket lens, I could diſcern that the point of the gold was expoſed.

The ſucceſs of this method exceeding my expectations, I coated ſeveral wires in the ſame manner, and found, that when ſparks from the conductors before-mentioned were made to paſs through water, by means of a point ſo guarded, a ſpark paſſing to the diſtance of $\frac{1}{8}$ of an inch would decompoſe water, when the point expoſed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I eſtimated at $\frac{1}{1500}$, a ſucceſſion of ſparks $\frac{1}{20}$ of an inch in length, afforded a current of ſmall bubbles of air.

I have

I have ſince found, that the ſame apparatus will decompoſe water, with a wire $\frac{1}{40}$ of an inch diameter, coated in the manner before deſcribed, if the ſpark from the prime conductor paſſes to the diſtance of $\frac{4}{10}$ of an inch of air.

Experiment 7. In order to try how far the ſtrength of the electric ſpark might be reduced by proportional diminution of the extremity of the wire, I paſſed a ſolution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold, lining the inner ſurface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the ſubſtance of the glaſs.

When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occaſion a ſtream of very ſmall bubbles to riſe from the extremity of the gold, although the wire, by which it communicated with the poſitive or negative conductor, was placed in abſolute contact with them. Hence it appears, that decompoſition of water may take place by common electricity, as well as by the electric pile, although no diſcernible ſparks are produced.

The appearance of two currents of air may alſo be imitated, by occaſioning the electricity to paſs by fine points of communication on both ſides of the water; but, in fact, the reſemblance is not complete; for, in every way in which I have tried it, I obſerved that each wire gave both oxigen and hidrogen gas, inſtead of their being formed ſeparately, as by the electric pile.

I am inclined to attribute the difference in this reſpect to the greater intenſity with which it is neceſſary to employ common electricity; for, that poſitive and negative electricity, ſo excited, have each the ſame chemical power as they are obſerved to have in the electric pile, may be aſcertained by other means.

In the precipitation of copper by ſilver, an inſtance of de-oxidation (or phlogiſtication) by negative electricity has been mentioned; the oxidating power of poſitive electricity may be alſo proved, by its effect on vegetable blue colours.

Experiment 8. Having coloured a card with a ſtrong infuſion of litmus, I paſſed a current of electric ſparks along it, by means of two fine gold points, touching it at the diſtance of an inch from each other. The effect, as in other caſes, depending on the ſmallneſs of the quantity of water, was moſt diſcernible when the card was nearly dry. In this ſtate a very few turns of the machine were ſufficient to occaſion a redneſs at the poſitive wire, very manifeſt to the naked eye. The negative wire, being afterwards placed on the ſame ſpot, ſoon reſtored it to its original blue colour.

By Mr. Volta's apparatus the ſame effects are produced in a much leſs time.

Beſide the ſimilarity which has thus been traced between the effects of electricity excited by the common machine, and thoſe obſerved from the electric pile, I think it appears alſo probable, that they originate from the ſame ſource.

With regard to the latter, its power is now known to depend on oxidation; ſo alſo does the excitement in the former appear very much to depend on the ſame proceſs; for,

Experiment 9. I have found that, by using an amalgam of silver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin, on the contrary affords a good degree of excitement. Zinc acts still better; but the best amalgam is made with both tin and zinc, a mixture which is more easily oxidated than either metal separately.

Experiment 10. But, as a farther trial whether oxidation assists in the production of electricity, I mounted a small cylinder, with its cushion and conductor, in a vessel so contrived, that I could at pleasure change the contained air.

After trying the degree of excitement in common air, I substituted carbonic gas, and found that the excitement was immediately destroyed, but that it returned upon re-admission of atmospheric air.

In conformity to this hypothesis, we find that the metal oxidated is, in each case, in a similar state of electricity; for the cushion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and in the same manner, zinc, oxidated by the accumulated power of an electric pile, or simply by action of an acid, is also negative.

This similarity in the means by which both electricity and galvanism appear to be excited, in addition to the resemblance that has been traced between their effects, shews that they are both essentially the same, and confirms an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in much larger quantity.

V.

An Account of some Galvanic Combinations, formed by the Arrangement of single Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. HUMPHRY DAVY, *Lecturer on Chemistry in the Royal Institution* *.

I. ALL the galvanic combinations analogous to the new apparatus of Mr. Volta, which have been heretofore described by experimentalists, consist (as far as my knowledge extends) of series containing at least two metallic substances, or one metal and charcoal, and a stratum of fluid. And it has been generally supposed, that their agencies are, in some measure, connected with the different powers of the metals to conduct electricity. But I have found that an accumulation of Galvanic influence, exactly similar to the accumulation in the common pile, may be produced by the arrangement of single metallic plates, or arcs, with different strata of fluids.

The train of reasoning which led to the discovery of this fact, was produced by the observation of some phænomena relating to the connection of chemical changes with the evolution of Galvanic power.

* Philosophical Transactions, 1801.

It

It appeared, in several experiments, that series of double metallic plates, incapable of acting as Galvanic combinations, when arranged in the proper order, with portions of water, were readily made to produce Galvanic effects, by being alternated with acids, or other fluids capable of oxidating one only of the metals of the series. Thus, double plates, composed of silver and gold, (metals which have been supposed to differ very little in their powers of conducting electrity,) produced Galvanic action, when placed in contact, in the common order, with cloths moistened in diluted nitric acid. And copper and silver acted powerfully with nitrate of mercury.

These facts induced me to suppose, that the alternation of two metallic bodies with fluids, was essential to the production of accumulated Galvanic influence, only so far as it furnished two conducting surfaces of different degrees of oxidability; and that this production would take place, if single metallic plates could be connected together by different fluids, in such a manner that one of their surfaces only should undergo oxidation, the arrangement being regular.

On this supposition, I made a number of experiments on different arrangements of single metals and fluids; and, after many various processes, I was enabled to ascertain, that many of these arrangements could be made active, not only when oxidations, but likewise when other chemical changes were going on in some of their parts.

In describing the different Galvanic combinations formed by single metallic plates and fluids, I shall divide them into three classes, following, in the arrangement, the order of time with regard to discovery.

II. The first and most feeble class is composed, whenever single metallic plates, or arcs, are arranged in such a manner that two of their surfaces, or ends opposite to each other, are in contact with different fluids, one capable, and the other incapable, of oxidating the metal. In this case, if the series are numerous, and in regular alternation, Galvanic influence will be accumulated, analogous, in all its effects, to the influence of the common pile.

Tin, zinc, and some other easily oxidable metals, act most powerfully in this class of combinations.

If pieces of polished tin, about an inch square, and $\frac{1}{20}$ of an inch thick, be connected with woollen cloths of the same size, (moistened, some in water, and some in diluted nitrous acid,) in the following order, tin, acid, water, and so on, till twenty series are put together, a feeble Galvanic battery will be formed, capable of acting weakly on the organs of sense, and of slowly producing the common appearances in water; the wire from the oxidating surface of the plates evolving hidrogen; and the wire from the non-oxidating surface (when of silver) depositing oxide.

In all cases, when the batteries of the first class are erected perpendicularly, the cloth, moistened in acid must be placed under the cloth moistened in water; and, in this arrangement, as the acid is specifically heavier than water, little or no mixture of the fluids will take place.

When

When zinc is employed, on account of its rapid oxidation in water containing atmofpheric air, three cloths fhould be ufed; the firft moiftened in weak folution of fulphuret of pot-afh, (which is poffeffed of no power of action upon zinc, and which prevents it from acting upon the water;) the fecond moiftened in a folution of fulphate of pot-afh, of greater fpecific gravity than the folution of fulphuret; and the third wetted in an oxidating fluid fpecifically heavier than either of the folutions. In this cafe, if the order be as follows, zinc, oxidating folution, folution of fulphate of pot-afh, folution of fulphuret of pot-afh, very little mixture of the fluids, or chemical action between them, will take place: and an alternation of twelve feries of this kind, forms a battery capable of producing fenfible effects.

III. The fecond clafs of GALVANIC combinations with fingle plates is formed, when plates, or arcs, compofed of a metallic fubftance capable of acting upon fulphurated hidrogen, or upon fulphurets diffolved in water, are formed into feries, with portions of a folution of fulphuret of pot-afh, and water, in fuch a manner that one fide of every plate, or arc, is in contact with water, whilft the oppofite fide is acted on by the folution of fulphuret. Under thefe circumftances, when the alternation is regular, and the number of feries fufficiently great, Galvanic power is evolved; and water, placed in the circuit with filver wires, is acted on; oxide being depofited on the wire connected with the fide of the plate undergoing chemical alteration, whilft hidrogen is evolved from the fide in contact with water.

Silver, copper, and lead, are each capable of forming this combination. Plates made from either of thofe metals, may be arranged with cloths, (moiftened, fome in water, and others in folution of fulphuret of pot-afh,) in the following order, metal, cloth moiftened in fulphuret of pot-afh, cloth moiftened in water, and fo on.

Eight feries will produce fenfible effects; and the wire from the top of the pile produces oxide.

Copper is more active, in this clafs of batteries, than filver: and filver more active than lead.

IV. The third and moft powerful clafs of Galvanic batteries, conftructed with fluids and fingle metals, is formed, when metallic fubftances oxidable in acids, and capable of acting on folutions of fulphurets, are connected, as plates, with oxidating fluids and folutions of fulphuret of pot-afh, in fuch a manner that the oppofite fides of every plate may be undergoing different chemical changes; the mode of alternation being regular.

The fame metals that act in the fecond clafs, may be ufed in the third clafs; and the order of their powers is fimilar. The pile may be erected in the fame manner as the pile with zinc in the firft clafs; the cloths moiftened in acid being feparated from thofe moiftened in folution of fulphuret, by a third cloth, foaked in folution of fulphate of pot-afh.

Three plates of copper, or filver, arranged in this manner, in the juft order, produce fenfible effects; and twelve or thirteen feries are capable of giving weak fhocks, and of rapidly producing gas and oxide in water; the wire connected with the oxidating end of the

the apparatus evolving hidrogen ; and the wire attached to the end acting on the ſulphuret, depoſiting oxide when compoſed of ſilver, and generating oxigen when of gold.

V. In all the ſingle metallic piles conſtructed with cloths, the action is very tranſient: the decompoſition of the acids, and of the ſulphurets, is generally completed in a few minutes; and, in conſequence, the Galvanic influence ceaſes to be evolved. The arrangement of all the different ſeries may, however, (by means of an apparatus conſtructed after the ideas of Count Rumford,) be made in ſuch a manner as to give conſiderable permanency to their effects. This apparatus is a box, covered with cement incapable of conducting electricity, and compoſed of three pieces of mahogany, each containing grooves capable of receiving the edges of the different plates proper for compoſing the ſeries. One half of theſe plates muſt be compoſed of horn, or glaſs, and the other half of metallic ſubſtances; and the conductors of electricity, and the non-conductors, muſt be alternately cemented into the grooves, ſo as to form water-tight cells.

When the apparatus is uſed, theſe cells are filled, in the Galvanic order, with different ſolutions, according to the claſs of the combination; and connected in pairs with each other, by ſlips of moiſtened cloth, carried over the non-conducting plates.

A combination of fifty copper-plates, arranged in this manner, with weak ſolutions of nitrous acid, or nitrate of ammoniac, and ſulphuret of pot-aſh, gives pretty ſtrong ſhocks, rapidly evolves gas from water, and affects the condenſing electrometer.

It does not loſe its power of action for many hours; and, when this power is loſt, it may be reſtored by the addition of ſmall quantities of concentrated ſolutions of the proper chemical agents to the fluids in the different cells.

From two experiments made on copper and ſilver, it would appear, that the ſingle metallic batteries act equally well, when the metals made uſe of are ſlightly alloyed, and when they are in a ſtate of purity.

VI.

Obſervations and Experiments undertaken with a View to determine the Quantity of Sulphur contained in Sulphuric Acid; and of this latter contained in Sulphates in general. By* RICHARD CHENEVIX, *Eſq. F. R. S. and M. R. I. A.*

IN a paper which I had the honour to preſent to the Royal Society of London, and the ſubject of which was the analyſis of ſome arſeniates of copper, and of iron, I had occaſion, in examining many pyrites, matrices of thoſe ores, to remark the very great inequality which prevailed in the reſults of repeated experiments, made with a view to determine the proportion of ſulphur. But I ſoon perceived, that the inaccuracy was cauſed by a partial combuſtion and acidification of the radical, through the means of the nitric acid, employed to diſſolve the ore.

* Iriſh Academy, 1801.

Having

Having therefore, in the ufual manner, afcertained what quantity of that ingredient remained untouched, I was forced to feek the reft of it in the liquor, which had wafhed the various precipitates. To obtain it, I poured a folution of nitrate of barytes into thofe wafhings, when all the other fubftances had been carefully feparated, and was thereby enabled to precipitate, in a ftate of purity, the fulphate of barytes, formed by that earth, and by the portion of fulphur originally acidified, in the firft treatment of the ore by nitric acid. To come at the knowledge of the proportion of fulphur, contained in a given quantity of fulphate of barytes, I had recourfe, in the firft inftance, to the quantity of fulphur, faid by Lavoifier to be contained in fulphuric acid, and, in the next, to the proportions of the latter, announced in the fynoptic tables of Fourcroy, as entering into the compofition of fulphate of barytes. According to the former of thofe chymifts, 100 parts of fulphuric acid contain 71 of fulphur and 29 of oxigen; and again, in adopting the proportions of the latter, we have 33 per cent. of acid, on fulphate of barytes. But if 100 contain 71 of fulphur, 33 muft contain 23.43. Confequently, for every 100 parts of fulphate of barytes, I was to allow 23.43 of fulphur. But, by the refults of my analyfes, I had fuch quantities of fulphate of barytes, as induced me to doubt the accuracy of one or other of the ftatements, by which I eftimated the quantity of real fulphur contained in the ore.

No perfon is better acquainted than our celebrated Prefident, with the many difficulties that occur in the analyfis of falts in general; particularly with regard to the quantity of real acid they may contain. It has been a work of trouble to the ableft chymifts, and they have not always agreed in their refults. The proportions, announced by Fourcroy, may therefore be doubted, in common with thofe of the other learned operators, to whom I have alluded.

The real quantity of acid, produced by the combuftion of any acidifiable bafis, can be determined by one or other of the following methods only: by direct combination, in fome falt, the proportions of which are are already known; or by obtaining, in a ftate perfectly free from water, the acid refulting from fuch combuftion. To the former method, the general objections againft all analyfes of falts muft apply. The latter is ftill more defective. It is by no means certain, that we have ever yet obtained any acid, in a ftate of perfect ficcity, unlefs we except the phofphoric and the arfenic; for even the cryftallized vegetable acids retain a portion of water in their cryftallization. It is not that I abfolutely deny our having obtained them fo; but I fay merely, that we have no proof. It would indeed be fetting narrow bounds to the perfection of nature, to affert, that no combuftible body could, when faturated with oxigen, affume of itfelf, the ftate of liquidity; or that the oxide of the particular fubftance, called hidrogen, muft be prefent to confer that property. Doubtlefs, fulphuric acid may, as well as water, contain, in itfelf, fo juft a proportion of fpecific heat, as to remain liquid at the temperature of our globe, and under the preffure of our atmofphere. But, both water and fulphuric acid being eafily volatilized, and having a powerful affinity for each other, it is not eafy, if even poffible, by diftillation, to feparate them with fufficient accuracy, in experiments of delicate inquiry. A fecond fource of

of error, therefore, remained open in this branch of the calculation, which gave the proportion of 23.43 of fulphur, as that contained in 100 parts of fulphate of barytes.

However, before I would allow myfelf to call in queftion fuch authorities as thofe I have quoted, I inftituted the following experiments: in a tubulated glafs retort, I put 100 parts of purified fulphur, and poured upon them ftrong nitric acid. A quilled receiver, plunging into a Woulfe's apparatus, was adapted to the retort; and, all being well luted, I proceeded to diftil. The liquor, which came over, was poured back feveral times upon the fulphur, until the whole was diffolved. The water, which had come over, and that, through which the nitrous gas, produced during the operation, had paffed, were effayed for fulphureous acid, and no traces of it could be found. No fulphur had been volatilized; therefore no fufpicion could remain, that all was not converted into fulphuric acid. The liquors, which were in the various parts of the apparatus, were united; and to them was added a fufficient quantity of nitrate of barytes. The whole was evaporated gently; becaufe, though I am well acquainted with the very little folubility of fulphate of barytes, I well know that nitric acid will retain a fmall portion of it, particularly when formed in a liquor where that acid abounds. In a firft experiment, I obtained 694 from 100 of fulphur; in a fecond, 348 from 50; and in a third, 347 from the fame quantity. But the fimple rule of three reduced thefe quantities to 14.6, or 14.4 per cent. of fulphur, contained in fulphate of barytes; a difference wholly to be neglected. If, therefore we take 14.5 as the average, for the quantity of fulphur contained in 100 parts of fulphate of barytes, we fhall not be far from the truth. From the accordance of thefe experiments, repeated and varied, I had now no doubt, but concerning the fource where I was to feek the error, which gave 23.43 as the juft proportion.

To afcertain this point, I operated in the following manner: I prepared fome lime, as pure, I believe, as chymical means can procure it. I digefted white marble in muriatic acid; and, by leaving an excefs of the earth, was certain, that by the fuperior affinity of lime for that acid, nothing elfe had been taken up. Upon trying the folution with ammonia, no precipitate took place. By means of carbonate of pot-afh, I feparated the lime in the ftate of carbonate; and, after well wafhing the precipitate, expofed it in a platina-crucible to a violent heat, till the weight no longer diminifhed. I am acquainted with no more efficacious method to prepare lime, fit for the delicate purpofes of fcientific chymiftry.

One hundred parts of this lime were diffolved in dilute muriatic acid, in the fame platina-crucible, previoufly weighed; and then fulphuric acid was added in fufficient quantity. Sulphate of lime was precipitated; and the veffel was expofed to a heat, at firft gentle, to evaporate the liquor; and then, by degrees, raifed to a temperature, which could expel every thing but the combined fulphuric acid, and leave the fulphate of lime completely calcined. The crucible with the falt was then weighed, and the augmentation was 76.— It appears to me, that if we admit (and I fee no reafon that we fhould not admit it) that calcined lime and calcined fulphate of lime are wholly exempt from water, it muft be clear, that the 76 additional weight were fulphuric acid; and, that the fulphuric acid muft

in

in this ſtate, more than in any other, approach nearer to what may be termed, abſolutely real acid. One hundred parts of calcined ſulphate of lime contain therefore

Lime - - - - -	57
Sulphuric acid - - - - -	43
	100

By the former experiments (thoſe made upon ſulphur converted into acid, and then united to barytes) we had the quantity of ſulphur, contained in ſulphate of barytes. By the latter (thoſe made by directly combining lime with ſulphuric acid) we had the proportion of real acid, contained in calcined ſulphate of lime. Conſequently, by knowing the ratio, that ſulphate of barytes bears to ſulphate of lime, with regard to the acid in each, we ſhall arrive at the knowledge of the quantity of ſulphur contained in real ſulphuric acid. For this purpoſe, I attempted to diſſolve in water, 100 parts of ſulphate of lime. But finding, in this method of proceeding, a conſiderable inconvenience ariſing from the great quantity of liquor, neceſſary to effect the ſolution of that ſalt, I had recourſe to the following expedient. Upon 100 grains of calcined ſulphate of lime, I poured ſome oxalic acid, which attracts the baſis with an affinity ſuperior to that exerciſed by ſulphuric acid. Oxalate of lime was here formed; but oxalate of lime is ſoluble in a very ſmall exceſs of any acid. A little muriatic acid operated a complete ſolution; and thus a great quantity of ſulphate of lime required but little water to diſſolve it. Into this liquor, muriate of barytes was poured, and ſuffered to remain ſome time, gently heated. By theſe means, any oxalate of barytes, that might have been formed, was retained in ſolution by the original exceſs of acid; and the intire quantity of ſulphate of barytes was depoſited. Of the exactneſs of all theſe methods which I uſed, as the inſtruments by which I aſcertained theſe reſults, I convinced myſelf by various preliminary experiments. After the uſual filtration, waſhing and drying at the gentle heat of a ſand-bath, I obtained in one experiment 185, in another 183, and laſtly, in another 180. This difference does not exceed the limits of what all perſons, converſant in analytic chymiſtry, will allow to experiments of this nature. We may therefore take 183 as the mean proportion; conſequently we ſhall ſay, that 183 of ſulphate of barytes contain the ſame quantity of ſulphuric acid, as 100 of ſulphate of lime; and 183 : 43 :: 100 : 23.5. Therefore 23.5 is the proportion of acid in 100 of ſulphate of barytes. But we have before ſeen, that 14.5 of ſulphur, acidified by nitric acid, form that portion of ſulphuric acid contained in 100 of ſulphate of barytes: viz. 23.5. We muſt now ſay, that 23.5 : 14.5 :: 100 : 61.5, and the fourth term will be the proportion of ſulphur = 61.5, which combined with 38.5 of oxigen will form 100 of real ſulphuric acid.

In neither of the proportions, whether it be of the acid contained in the ſalt, or of the combuſtible baſis contained in the acid, do I agree with the two chymiſts whom I have quoted. This juſtly excited ſome doubts in my mind, and led me to repeat my experiments. Nor ſhould I yet be thoroughly ſatisfied, if I could not, upon other grounds,

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than by ſuppoſing inaccuracy in them, account for the apparent differences. We muſt ever expect to ſee the errors of our predeceſſors corrected by men, much inferior in abilities; but who, by poſſeſſing more certain means, ſupply the want of genius and invention. At the time in which the experiments were made, that determined the proportion of 33 per cent. of ſulphuric acid in ſulphate of barytes, it was not known that we had never obtained any barytes pure; and that a conſiderable portion of carbonic acid reſiſted the action of every degree of heat that had been applied to carbonate of barytes. The fact was, I believe, firſt obſerved by Pelletier; but the method of avoiding the inconvenience was pointed out by Vauquelin. He decompoſes nitrate of barytes by fire, and a moderate degree of heat is ſufficient to expel all the acid and the water. The chymiſts I have mentioned, performed ſynthetic experiments, by combining, directly or indirectly, ſulphuric acid, and ſuch barytes as they imagined to be pure. The conſtant ſimilarity of their reſults is ſufficient to prove the accuracy of their operations; but working upon an impure ſubſtance, they muſt have been contented with a ſimilarity of error.

Three cauſes may exiſt which are capable of accounting for any variation, whether in *plus* or in *minus*, that might have appeared in the experiments, by which Lavoiſier determined the quantity of ſulphuric acid, obtained by the combuſtion of ſulphur in oxigen gas.

1ſt. A part of the ſulphur may be volatilized during combuſtion.

2d. All the ſulphur may not be converted into ſulphuric acid; but part may remain in the ſtate of ſulphureous acid.

3d. In rectifying, ſome acid may come over along with the water; or, *vice verſâ*, ſome water remain with the acid. Theſe conſiderations will excuſe me for having propoſed a doubt, where the authority of ſo great a man exiſts againſt the experiments which I have related.

The method which I had uſed to aſcertain the quantity of ſulphur in an ore, had been practiſed by Taſſaert, (*Annales de Chimie*, No. 82: Analyſis of Cobalt from Tunaberg) but he calculated according to the proportions of Lavoiſier and of Fourcroy. In another paper by Thenard, (*Annales de Chimie*, No. 96.) he ſtates the proportions of ſulphuric acid, obtained by treating ſulphur with nitric acid, to be

Sulphur - - - -	55.56
Oxigen - - - - -	44.44
	100.00

but in the extract given by Guyton in that No. of the *Annales de Chimie*, the mode of operation is not deſcribed. Calcined ſulphate of barytes is eſtimated in the ſame paper to contain

Barytes - - - -	74.82
Sulphuric acid - - - -	25.18
	100.00

which

which proportions are as near to what I had found as can be expected; for sulphate of barytes does not contain more than 3 per cent. of water of crystallization, and they must be deducted from the quintal.

Having determined with accuracy the proportion of acid in any insoluble sulphate, it is easy to proceed to the determination of that contained in any other sulphate. The docimastic art or analytic chymistry, in general, cannot however expect to derive such advantages from the knowledge of soluble salts, as of those, which, from their insolubility, may be used with accuracy in delicate experiments to determine the proportions of the constituent parts of bodies. But if, with this mode of operating, we compare the quantities of real acid, said (in those excellent tables with which Mr. Kirwan has enriched the science) to be contained in sulphuric acid of different specific gravities, each will serve as a proof of the validity of the other; and perhaps demonstrate, that sulphuric acid, without the intervention of water, may enjoy liquidity, at the temperature and pressure which act upon our globe.

VII.

Description of an Apparatus for Distilling, in which the Effects of Absorption are prevented without admitting the external Air. In a Letter from Mr. A. S. Burkitt.

To Mr. NICHOLSON.

SIR,

I HAVE taken the liberty of inclosing a drawing of an apparatus, to prevent the absorption which so frequently happens in distilling acids, in particular the nitrous; for which I have mostly used it with the desired effect.

Should it prove original, and meet your attention, you will confer a favour by giving it a place in your valuable Journal.

I am, SIR,

Your most obedient humble Servant,

A. S. BURKITT.

Fleet Street. Nov. 20, 1801.

Plate XVII. represents the apparatus; where A is a retort, B, the usual receiver, having a neck to receive the retort, and another from which the recurved tube F proceeds to the bottom of a second receiver C; and from this last vessel a communication is made in the well known manner with the apparatus of Woulfe D. The principal singularity and advantage of my apparatus consists in the valve E, constructed by placing a plate, or plano-convex lens upon the mouth of a smaller tube, inserted by grinding within F like the valve in

in North's apparatus, but with more water way; and this advantage I find in practice to be superior both in effect and cheapness to the ingenious tube of safety of Walter. That contrivance, so familiar to chemists, consists of a perpendicular tube with a small bason at top, and another reservoir or swell at bottom, in an ascending part of the same tube; and its action when duly charged with mercury is, 1. When the internal pressure increases, the mercury rises, till the whole column is supported, after which, upon any farther increase, the intire mass of mercury is suddenly blown into the upper reservoir, and gas is at once let out, till the spring is reduced nearly to that of the atmosphere; and 2. If there be absorption, the external air in all cases is very little resisted, but is suffered to enter the vessels.

In my apparatus the pressure during the transmission of elastic fluid through the valve is constant; being equal to the re-action of the liquids in the vessels C and D, and whatever other external force may be added by the well known means: and whenever by diminution of the fire, or other causes, the spring of the gases in B shall become less than the pressure of rhe external air, that space will become partly vacuous; but the valve will prevent any return of the fluid from C, so that the only effect will be, that the materials in A will give out their product somewhat more rapidly from the diminished re-action.

A. S. B.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Discovery of Comets.

IN the journal of the 8th of prairial the Notary Caigné announced, that he had 600 fr. to pay to any one who should discover a comet. On the morning of the 23d of Messidor, C. Pons, keeper of the observatory at Marseilles, discovered one, which three able astronomers of Paris also observed the same evening. The board of longitude, being requested to adjudge the prize, considered C. Pons as having a degree of priority, to which the three astronomers of Paris assented, and the prize was awarded to him. I am soliciting the government to offer a similar prize, to accelerate the progress of our knowledge of comets, the only branch of astronomy which is not in an advanced state. It is so easy, however, to find comets, that any one, without being an astronomer, may discover them when he pleases.

LALANDE.

Mémoires d'Agriculture, &c. Memoirs of Agriculture, and Rural and Domestic Economy, published by the Agricultural Society of the Department of the Seine, and printed by Order of the Prefect of the Department. Vol. I. Paris, 8vo. 447 p. 1 Plate. Price 4 fr.

At

AT the head of this volume are the order of the Prefect for printing it, the rules of the society, and a list of its ordinary and corresponding members, with an account of the society's labours: after which are the following memoirs —Instructions for preserving Wheat from Smut, by C. C. Cadet-de-Vaux, Parmentier, Saint.Genis, and Yvart.—Report of the Experiments of C. Houdart, Junr. on preparing and economising seed—On the means of draining various lands by simple and not expensive processes, by C. Chassiron. This memoir, which has been published separately, is preceded by some observations on the general system of inland navigation in France, and followed by a comparative table of the coal-mines worked in each department, and of those which require only navigable canals and rivers to enable them to be worked. The former are fifty-one in number; the latter, sixty. Their importance to those of the French manufactures that require fuel, our mechanic arts, and our agriculture, must be obvious, at a time when we are perhaps not sufficiently aware of the want of wood, with which we are threatened, from the great devastation of the forests in most parts of the republic; a subject which has been treated of by C. Lasterie in a separate memoir in this volume.—On the precise Signification of the terms *Agriculture* and *Rural Economy*, by C. Cels.—Some Reflections on the supposed number of Sheep in France, by C. Delong. The intention of this memoir is to excite inquiry concerning this important subject.—The advantages of nurseries on estates of a certain extent, in facilitating such annual plantations as may be suited to them, by C. Villele.—An interesting Account of the successful Amputation of the fore leg of a Cow, which had been fractured, by C. Chaumontel.—Experiments, Inquiries, and Observations on Elms, by C. Boucher; to which C. Denorgelles has added his Experiments for obtaining Alcohol from the sap of Vegetables.—On the product of different sorts of wheat that are cultivated, and the melioration of them; or the advantages that would result from inquiring what sorts of wheat yield the most bread; by C. Chancey.—On Rural Edifices, by C. Garnier-Deschénes.—On the Manner in which the Mountains in the Cevennes are fertilized, by C. Chaptal.—On the Means of rendering fit for use Wells that have been abandoned in consequence of the Mephitisation of the Soil, by C. Cadet-de-Vaux.—Observations on the working of Wines, by C. Creusé-Latouche.—On the Cultivation of the Sugar-Cane in the Caribbee Islands, and particularly of that of Otaheite, by C. Moreau-St. Mery.—On the Cultivation of the Sugar-Cane, by C. Cossigny.—On the Cinnamon-Tree of French Guiana, by C. le Blond.—The volume concludes with accounts of the lives of Cretté de Palluel, and T. Francis de Grace. *Mag. Encycl.*

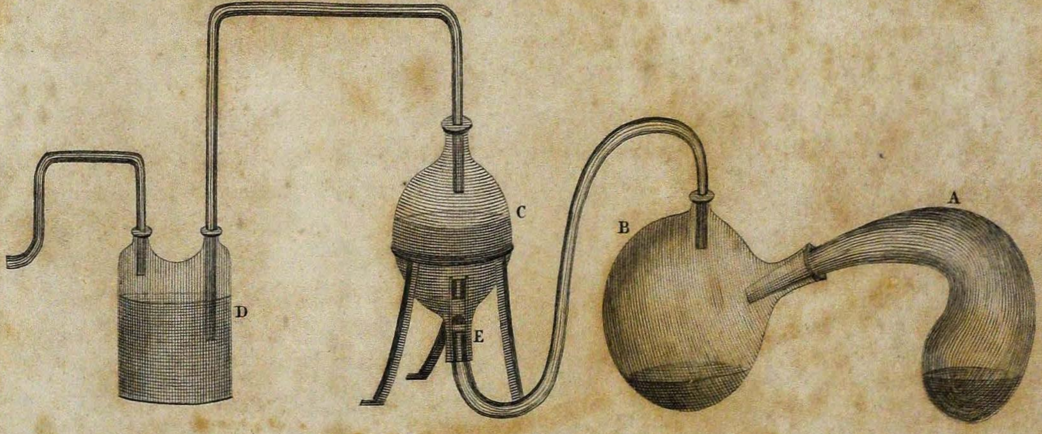
Histoire Naturelle des Mineraux, &c. The Natural History of Minerals; by E. M. L. Patrin, Associate of the National Institute; containing a Description of Minerals, and of their native Situation [Gite], *the Theory of their Formation, their Connection with Geology, or the History of the Earth, an Account of their Properties and Uses, their Chemical Analysis, &c.* 5 Vols. large 18mo, about 350 p. each, with 40 Plates. Price 12 fr. 50 c. plain; 18 fr. 50 c. with coloured plates; on fine paper, 5 fr. 50 c. more.

C. PATRIN

C. PATRIN has made himself known by several memoirs in the *Journal de Physique*, in 1788 and 1791, on the mines of Siberia, which he spent eight years in examining from the boundaries of Europe to the frontiers of China, and whence he has brought home a collection of minerals as valuable as it is ample. The object of this tour was to collect information concerning the structure of mountains in general, the formation of metallic veins, the relation that different mineral substances bear to each other, and the history of the terrestrial globe itself. It is particularly in this important point of view, that the author has treated of the history of minerals. This too was the object proposed by the illustrious Buffon; but that great man wanted the aid of observation, which genius cannot supply; and what he has said on the subject is built wholly on systems formed in the closet, and now considered only as splendid chimeras. To these different notions of general theory, the author has added all the particulars that he judged of most importance respecting the properties of the different mineral substances, and their uses in physic or the arts: every thing, in short, that can render them serviceable to man. He has carefully added the analyses made of them by the most celebrated modern chemists, which were altogether omitted by Buffon; though, indeed, had they not, what he could have given would have been of little use at present, from the amazing superiority the chemical operator has attained since his time. In this work is an essay on volcanoes, in which C. P. advances a new theory of these grand phenomena: and one thing shews, that the author has seen the operations of nature contemplated as they actually are; his having pointed out several discoveries as possible, which have been really made since his memoir was read at the Institute.

END OF THE FIFTH VOLUME.

Mr. Burkitt's arrangement of distillatory Vessels
to prevent the effects of absorption.



View of the basaltic Columns of Fairhead on the Coast of Ireland

Muston, sc. Rufsel Co.

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London, Dec. 1, 1801. *W. NICHOLSON*